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## THE COMPARATIVE VALUE OF VARIOUS CONCEPTIONS OF NERVOUS FUNCTION BASED ON MECHANICAL ANALOGIES\*

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The word "mechanical" in the title of this paper is used in its widest sense, as meaning anything machine-like. The advantage of mechanical analogies in our conceptions of the functioning of the nervous system consists in their forcing us to think in quantitative terms. We psychologists, and also the physiologists and zoologists who are interested in our problems, have been accustomed too long to think exclusively in qualitative terms. Imagine an engineer designing (and true understanding is equivalent to designing) a machine by thinking in such purely qualitative terms as this: the rise of temperature in the boiler is "instinctively" or "reflexly" followed by a rotation of the fly-wheel; each movement of the crank through one hundred and eighty degrees "associates itself" with one movement of the valve gear; the strokes of the piston-rod are "inhibited" by a raise of the damper lid on the flue, etc. The engineer can design a machine only by thinking of a fly-wheel of a definite shape and weight, of a piston-rod of a definite length, of a piston of a definite cross-section and length, of a boiler of a definite size, of a definite degree of temperature within it, of a definite quantity of coal, etc. We shall never understand the function of the nervous system unless we are willing to think of it, too, in quantitative terms. We can use quantitative terms equally well in either of two ways, by means of geometry, and by means of arithmetic and algebra. A quantity or a quantitative relation can be defined equally well in a graph and in an equation. A description of a nervous function which employs neither a graph nor an equation, but humors the reader who dislikes mathematics by confining itself to qualitative terms, is not worth the paper on which it is printed.

This problem has stood in the center of my interest for more than ten years, although I have expressed myself on it

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only during the last five. But others have been at work on this problem, too. You have undoubtedly heard of Uexküll. He does not, however, offer much of interest to the psychologist because he has devoted himself chiefly to the explanation of the behavior of such animals as insects and still lower forms, whose nervous systems are undeveloped in comparison with that of man. His mechanical analogies are, in my opinion, neither applicable to higher functions, as, for example, to learning,—the central fact of psychology,—nor are they simple enough for the demands of science, nor are they always easily translatable into anatomical and physiological terms. I agree, on the whole, with Jennings who expressed the following opinion: "It seems that, even for practical purposes, the author (Uexküll) has overestimated the value of a rather gross 'Anschaulichkeit.' The bringing in of machine-like structures,—tubes, valves, etc.,—that confessedly do not exist, seems rather to confuse than to aid the mind. It is not possible to conclude directly from the properties of the assumed machines as to what physiological properties one will find, for the parallelism is far from complete, so one must try to keep the system of machinery separate from the system of physiological facts; there are two systems to grasp instead of one."<sup>1</sup> Let me say at once that I did not derive any of my ideas on this subject from Uexküll.

Mr. Nathan A. Harvey, of Ypsilanti, Mich., has been working along similar lines. But the chief problem of psychology, that of learning, became a stumbling-block to him. He announces his renunciation in the following words: "There is one thing that is not perfectly plain in this scheme: No explanation can be thought of, at least by the present writer, that would account for the shifting of the dendrites in the process of attention. Why should the dendrites move? Here is a place where no answer can be given, and when a person points out that this involves some supernatural agency and cannot be accounted for in terms of pure physiology, we have to acknowledge that we see no way of accounting for it."<sup>2</sup>

Quite recently Mr. S. Bent Russell, an engineer of St. Louis, has offered some interesting ideas on this subject. The

<sup>1</sup> H. S. Jennings, *The Work of J. von Uexküll on the Physiology of Movements and Behavior*. *Jour. Comp. Neurol. and Psychol.*, XXIX, 1909, 330f.

<sup>2</sup> Nathan A. Harvey, *A Device by which Physiological Concepts may be employed in teaching Psychological Functions*, *Western Journal of Education*, IV., 1909, 182.

last part of this paper shall be devoted to a comparison of the functional concepts as published by Mr. Russell<sup>3</sup> and those published by myself in my book on the Fundamental Laws of Human Behavior (1911). In some respects I agree with Mr. Russell; in others I disagree. Whatever our differences may be, however, I wish to express the opinion that Mr. Russell has done science an invaluable service by the publication of the paper in question. He describes in all structural details a hydraulic machine which, if inserted in a steamship, would reveal among other possibilities the one described in the following tale.<sup>4</sup>

A canal, large and deep enough to carry a steamboat, has the shape of the numeral 8, or, more strictly speaking, of two circles touching each other. At the point of contact, where the waters belong to the one circle as well as to the other, a steamboat is floating, in the direction of the tangent of both circles. On the deck of the steamboat there are three levers, No. 1, No. 2, and No. 3. No. 1 starts the propeller driving the boat straight ahead. No. 2 starts a second propeller which, serving instead of the rudder, gives the boat a turning movement to the left. No. 3 starts a third propeller which gives the boat a turning movement to the right. On deck, before the three levers, stands one of those psychologists whom I criticized in a paper read before this association a year ago,<sup>5</sup> who believe that a machine can change its ways of working only if a ghost is kind enough to step in, disconnect something, and connect it with something else, —in other words, who believe that "experience" without a ghost is unthinkable. It is a fine Sunday morning, and our experimental psychologist begins his pleasure drive by pulling lever No. 1. But soon he observes that the boat is straight-way approaching the dividing point of the canals which threatens destruction. He pulls lever No. 2; the boat swings to the left and follows the circular route of the canal without touching the shore. After a while the opposite point of the circle is reached. It is lunch time. Our psychologist pushes both levers, No. 1 and No. 2, back, and the boat stops. After lunch, to continue his trip, he again pulls lever

<sup>3</sup> S. Bent Russell, A Practical Device to Simulate the Working of Nervous Discharges. *Journ. Animal Behavior*, III, 1913, 15-35.

<sup>4</sup> The tale is not given in Mr. Russell's paper, but, I hope, will appeal more strongly to the imagination than Mr. Russell's equivalent, but somewhat dry, description in engineering terms.

<sup>5</sup> Max Meyer, The Present Status of the Problem of the Relation between Mind and Body. *Journ. Philos. Psychol. and Sci. Meth.*, IX, 1912, 365-371.

No. 1. Suddenly he remembers that No. 1 serves the forward propeller, and looks ahead to see if he is dangerously near the shore. To his amazement he finds that the boat is following the circle properly. Not only is the forward propeller working; the propeller turning to the left is working too, although no one has pulled lever No. 2. The steamboat has an engine which "remembers." After some time the point of departure is reached. Lever No. 1 is pushed back, the boat stops in its original position, and our experimentalist goes home.

The following Sunday we find him again on deck. He pushes lever No. 1 and the boat starts. But the engine no longer "remembers." After so long an interval as a week it "forgets." The boat moves straight ahead. Our adventurer pulls lever No. 3; the boat swings to the right and follows the route of the other canal. Lunch time comes again. Both levers, No. 1 and No. 3, are pushed back, and the boat stops. After lunch the experimenter pulls lever No. 1. The engine "remembers." The boat does not go straight ahead, but follows the curving of the canal to the right. The forward propeller and also the propeller turning to the right both work, although lever No. 3 was not touched again. But after a week the engine "forgets" this experience, too. Nothing, however, prevents it from relearning.

Now, this is not altogether a fairy tale. This wonderful engine (which, after all, is no wonder) is described in sufficient detail in Mr. Russell's paper and, in summary form, it is spoken of on page 30 in the following words: "Figure 6 and table III. show a duplex converging gang. In this arrangement the first key rod (or sensory terminal) *SS* is known as the station key. *MR* and *ML* are opposite movements. If *SS* and *SL* are habitually struck in succession, except when *SS*, *SR* and *SL* are struck in succession, the device will become "trained" so that when *SS* is struck the movement *ML* will result. On the other hand if *SS* and *SR* are habitually struck in succession the key *SS* will when struck give the opposite movement *MR*." Nothing, except the limitations of your pocketbook, prevents you from having such a pleasure yacht built for your own use in vacation hours. You simply have to follow the specifications given by Mr. Russell in that part of his paper which is devoted to their exposition, beginning on page 21 with the heading "Operation" and ending on page 30 with the words just quoted.

In the pages from 31 to 35,—the closing section of his

paper,—Mr. Russell gives specifications for hydraulic machines capable of still other varieties of learning by experience, in fact, of most of those varieties which I have enumerated in my book on Human Behavior. And nowhere in his specifications is any ghost to be found. The specifications contain objective realities exclusively.

What, now, is the value to pure science of these pages, from 21 to 35, of Mr. Russell's paper? Of course, their value to the millionaire who desires to own a pleasure yacht which remembers and forgets its experiences, is obvious, but merely practical. Their value to pure science, as I see it, consists in the fact that we have here a demonstration of the possibility of an "organism," capable of learning and forgetting, which obeys no ghost whatsoever, but only the laws of mechanics. This possibility has been denied by those psychologists, led at present by William McDougall, who have written "Interactionism" on their standard. They might perhaps, in order to save their position, reply that S. Bent Russell has proved this possibility only for a mechanical, but not for a biological organism. But is this not a purely technical evasion of the point at issue? We have always accepted almost like a dogma,—and shall continue to do so,—that what is possible in "mechanics" (taking this word in a narrow sense as the science of matter and motion, but including hydromechanics) is possible in physics-chemistry, the larger field of science; but not the reverse. And we further believe that what is possible in physics-chemistry, is possible in biology; although the reverse may not be true. If it is proved that a mechanical organism can learn and forget without the interaction of a ghost, we have no right to assert that a biological organism can not.

Is Mr. Russell's demonstration of a learning and forgetting machine also valuable to science in suggesting new experimental work in physiology, anatomy, zoology? Here, unfortunately, its value appears very limited. The objections raised by Jennings against much of the theoretical work of Uexküll, apply with equal strength to this of Russell. What suggestions does the neurologist receive from a multitude of terms like these: spur valve, pawl, pawl spring, ratchet valve, rocking finger, finger lever, plunger, dash pot barrel, balanced slide valve, key rod, bell crank, suspender link, coupling gang, etc.? I am far from asserting that none of these terms is translatable into neurological terms; but I do not doubt that most of them are untranslatable.

Let us now turn to a consideration of the first pages, not

referred to as yet, from 15 to 20, of Mr. Russell's paper. I propose to compare very briefly our quantitative concepts of nervous function. As previously explained, the geometrical drawings are an essential part of this concept. I represent the nervous system as consisting throughout of arches, parallel or superposed, each arch consisting of a rising, a connecting, and a falling line. I have also devised a consistent method of lettering; but this concerns us less, at present. Each line represents a neuron. This does not mean that in the anatomy of an animal it may not denote several neurons. But no fewer than three can compose an arch. It appears immediately to the eye that some arches are reflex arches, others, the higher arches, are "nerve centers." Regarding the resistance of each neuron as originally the same, it appears immediately to the eye that certain points (sensory and motor) are "corresponding," that is, connected by a nervous path of the smallest possible (length, and therefore) resistance, and that other points are connected only by way of superposed arches, by paths of greater resistance,—how much greater is again immediately clear to the eye.

Mr. Russell uses a different kind of diagram. Where I use an arch of three lines as representing a reflex arc, he uses an angle of two lines. Where I use an arch of three lines as representing a superposed nervous arc, he uses a diagram of four lines. He seems to prefer this method of drawing the diagram on account of its showing all the sensory points on one (the upper) side of the diagram, and all the motor points on another (the right) side. I am not certain that this is an advantage. But this method, in my opinion, has the decided disadvantage that one cannot build up systematically an unlimited number of higher and higher nerve centers without making the figure confusing, for all the higher centers of different level would be mixed up among themselves and with the reflex arcs. In my method all the arcs representing the same nervous level, that is, the same closeness or remoteness of connection of a sensory with a motor point, appear clearly to the eye as standing on the same level in the diagram. Having counted the number of neurons from one of these arcs to the sensory or motor periphery, we know it for all other arcs on the same level.' Counting, indeed, is not necessary even in the first place, since my method of lettering gives the answer without counting. It is not strange, then, that Mr. Russell, in his stream diagrams, restricts himself to one level above the reflex arcs. That the application to animal behavior demands the removal of such

a limit of the number of levels, I have shown in my book, for example, on page 44.

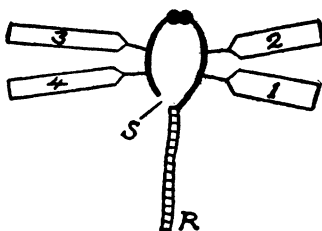
Mr. Russell seems to believe (p. 19, and also the last paragraph of his paper) that he has given an explanation of the mechanism of the association of ideas. I do not concede this. In order to give such an explanation, it is necessary to make first a definite assumption with respect to the nervous correlate of a mental state. I have made such an assumption (on pages 233 ff.) and I have given the explanation: Mr. Russell has not.

A similar remark may be made with respect to what Mr. Russell says on page 33 about satisfaction: "This mechanism . . . illustrates . . . satisfaction, showing how associations may be reinforced or stamped in." I have stated the nervous correlate of feeling on pp. 238-239 of my book.

Mr. Russell, throughout his paper, uses the term "susceptibility" in a sense entirely different from that in which I have consistently used it. I mean by (positive and negative) susceptibility (p. 115) of a neuron its ability to respond to a flux by an increase of conductivity, to lack of function by a gradual return to the former conductivity. Mr. Russell, however, does not use the word susceptibility in this sense of "being capable of a gain or loss in conductivity," but in the sense of a synonym for conductivity itself. He uses, in addition, capacity as a synonym for the same term, conductivity. It is my opinion that no synonyms are needed for conductivity, and that the use of either susceptibility or capacity in this sense is confusing.

To make plain the different ways in which the nervous processes streaming through the diagram influence each other, Mr. Russell uses the analogy of a hydraulic machine consisting of fifty or a hundred parts, of some of which I have quoted the names. These parts of the machine, essential for its functioning, mean practically nothing to the student of neurology, suggest no new experiment. Let us compare the mechanical analogies which I have used. I assume that the point of contact between the ends of two conductors serves (normally) as a one-way valve (called check valve by the engineer). The translation into neurological terms is simple. We assume that the so-called synapse, much abused in psychological theory, when normally functioning, permits the passage of the nervous excitation in one direction only. I further make an assumption about the relative susceptibility in lower and higher arches, which hardly needs a translation into, but is practically stated in, neurological terms. This assumption

directly refers to certain known facts of neurology and animal behavior<sup>6</sup> and suggests further experimental work in these sciences. I also introduce (and this is practically all the mechanical analogism which I use) the two concepts of deflection and overflow. The latter means this. Think of a metal wire of a given electrical resistance. If its temperature rises, its resistance becomes greater; and the rise of temperature may be due to the current itself. Suppose something similar holds true for a neuron carrying a nervous excitation,—there you have the translation, simple and direct, into neurological terms. The consequence would be of the nature of the function which I have called overflow, that is, certain rather long paths would carry a greater flux than was to be expected from the original computation of the resistances. And this plays a great rôle in the explanation of what I call instinct.



Deflection of a stream by another, stronger stream, drawing the weaker one into itself, is easily demonstrated by means of an apparatus which anyone can build out of two glass T's, a glass jet, a few feet of rubber tubing and a bottle of ink, the whole to be connected with the water faucet. I do demonstrate this, for the sake of "Anschaulichkeit," to use Uexküll's phrase, every semester to my students. The translation into neurological terms is self-evident, if we are willing to think of the nervous process as a streaming of ions and of the nervous architecture as corresponding to the general features of my diagrams. Let no one think that the experimental proof of such a nervous "deflection" is a doubtful possibility of a remote future. The deflection was demonstrated ten years ago by a brilliant experiment of Uexküll.<sup>7</sup> In our figure is seen the anterior part of the nervous system of a worm. It is artificially severed at S. 1,

<sup>6</sup> Cf. *Human Behavior*, 113-115.

<sup>7</sup> J. von Uexküll, *Ergebnisse der Physiologie*, III, 2, 1904, 10.

2, 3, and 4 are muscles which have a connection of low resistance with the ventral cord, but a high resistance connection with the head ganglion. If the head ganglion is stimulated, the muscles 1, 2, 3, and 4 respond, but weakly, owing to the high resistance of the nervous paths. If at the same time the ventral cord is stimulated at *R*, 1 and 2 respond strongly, of course, but 3 and 4 respond still more weakly. And if the stimulation of the ventral cord is made very strong, 3 and 4 no longer respond to simultaneous stimulation of the head ganglion at all. That is, the whole nervous process coming from the head ganglion, instead of partly going in the direction of 3 and 4, is now forced to join entirely the stronger nervous process coming from *R* and having its motor outlet in 1 and 2.

If you prefer a demonstration taken from the study of behavior, to an experiment made on a nerve preparation, think of all those facts talked of in the psychological text-books under the head of "attention." Why is it that the enthusiastic chess-player, walking home and at the same time occupied with his next move, suddenly stops in the middle of the street? Because the complex nervous process of preparing for his next move is so much stronger that it deflects the weaker nervous process which controls the muscles of the legs and thus keeps it from sending to these muscles the proper innervations.

I believe, then, to be justified in saying: The mechanical analogies which I have used are few. They are simple. Their significance is comprehensive. And they are easily translatable into physiological terms.